

STANFORD UNIVERSITY MEDICAL CENTER

DEPARTMENT OF GENETICS

March 31, 1977

Dr. Harriet Zuckerman
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Dear Harriet,

Your letter of March 28 arrived just about simultaneously with Graubard's copy-edited version of our manuscript; but I have really not had a chance to look at that.

You did bring up some information about Norton Zinder that I hope I can clarify. I do know that at the time the Nobel Prize was awarded to Beadle, Tatum and myself in 1958 that the same press release that announced Tatum's part in it also highlighted Norton Zinder's role in the work that taken together was the justification for my own part of the award. This would suggest that a number of people believed and, in part, persuaded Norton that he might have been justly entitled to some share in that particular recognition. Since, as you well know, there is a limitation to three recipients in a given award, the matter had to be settled quite arbitrarily in Stockholm; and needless to say I had no part in it. As you well know, just where to draw the line is the most difficult task and some sense of the arbitrariness of that particular discrimination must have contributed to my mood in the writing of my brief formal speech at the celebration. I am quite confident that Norton has no overt feeling of resentment to me about this nor do I believe that he has any substantial cause for complaint at my own hands. There was no question, that he should be the senior author of the principal paper announcing transduction that appeared in the Journal of Bacteriology in 1952. But the circumstances of the award cannot but help have aroused some ambivalent feelings and I suspect that is what the whole story is about.

Some years ago, I encouraged Norton to write down his own account of the details of that discovery without asking that I or anyone else take a look at it, and I hope that he may have that available to offer at least his own perspective.

What I don't understand is the way in which these considerations connect with "certain sections of our paper." I first met Norton when he came to Wisconsin as a graduate student, at Francis Ryan's recommendation, in July 1948. I had already done some work with Salmonella and explicitly suggested to Norton that he look into the extension of the search for recombination in Salmonella along the lines that had just recently been worked out in E. coli. That led us a merry chase, which I think is reasonably well documented in the published papers, before we realized that we had a totally new phenomenon on our hands and the virus was the actual vehicle transmission of genetic information. I played devil's advocate for quite some time before being willing to accept that this hypothesis had been clearly corroborated in preference to others that we were also toying with. I doubt very much whether it would be possible to reconstruct the day to day or week to week evolution of that concept although there is a snapshot of the evolving doctrine in the 1951 Cold Spring Harbor Symposium: there we talk about the phage eliciting the FA, the filtrable agent, and had

not yet reached the conclusion that it was the phage particle itself.

But all of this postdates the framework of the discussion in our paper by several years. So I am still in doubt what you had in mind about your concern about the intersection of transduction with our story.

I will be starting to look at the Graubard revision in the next couple of days and expect one or the other of use to initiate some further critical dialogue.

Bob Noyce has said no to us. I don't know where we are with Littlefield, but I predict that Pierpont, who was more or less firmly the backup candidate, will be asked next.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

P.S. Trying to catch up with some backlogged mail, here are a couple of items: On March 4, you sent me a questionnaire which was obviously very carefully worked out. But I had to say that some of the questions stimulated a blizzard of further traffic and that introduces a certain chance element in just how the questions will be answered. This is an old story, and you may well be probing how your respondents interpret the question at the same time as you are analyzing their reply. But you may want to know that they may have evoked images other than some of those that you possibly had in mind yourself. For example under III. Academic Standards, #13; 13a. and 13c. both refer to "the quality of a proposal" with the implication that this can be judged in the abstract without to the personality or the reputation of the author or the past work. Have you asked yourself what you mean by the phrase? Is it the literary quality of the piece of paper; the diligence and critical acuity with which past work has been examined. Is it the clarity with which planned future experiments are described. Or is it the expectation of a scientifically important outcome. Given that inherent ambiguity, one might have posed the question whether grants should be awarded on the criterion of expected outcome, all things considered, or the attributes of the proposal as a document disregarding those expectations, or some mixture of both. Obviously this is not the place to discuss the substance of the question, *unwar* but I think there is an issue as to how the question itself may be perceived.

Under question 16 and 17, there are also problematics about what constitutes plagiarism. I guess I have never heard of a case myself where actual scientific results were stolen and published by someone else; perhaps Pace, the claims of people like Schatz. I guess it has not been unusual that some idea that I had voiced is elaborated by someone else, who then may even have run a race to achieve priority in publication. So the issue is whether the later type of event is plagiarism. The same consideration applies in #18 about the meaning of the term "results." *Pace*

Under question 46, the subdivision of the biological sciences is confusing. For a number of people there may be an almost random allocation within molecular

and cellular biology which of the five subcategories they would choose. Virology and microbiology overlap -- one might better have asked whether one's principal target organism was a virus, another prokaryote, or a eukaryote. The questions of biochemistry or genetics are orthogonal to the preferred research material, and immunology is another wrinkle that cuts across that. So I suspect that the fine structure there is going to end up not being terribly helpful for any other analysis and may well be ignored. There was also another category called basic health sciences that will add to the confusion.

Question 53 -- the explanation under 55 should be pointed to immediately under 53 as defining the range of what is meant by consulting. Question 98 -- do you mean confidence that the manager will get ahead in the world, will do a good technical job within the framework of his own institution, or will be responsible to broader social needs?

115 f.

One-fifteen-f -- I would have turned the alternative around, in ^{Popperian} ~~Paperian~~ spirit.

Well, altogether it sounds like great fun.

Sincerely yours,

Joshua Lederberg
Professor of Genetics